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Original Articles.

ALCOHOL AGAIN: A CONSIDERATION OF RECENT MISSTATEMENTS OF ITS PHYSIOLOGICAL ACTION.

BY JOSEPH W. WARREN, M.D.,
Assistant in Physiology in the Medical School of Harvard University.

THE friends of temperance, falsely so-called, since total abstinence, and usually prohibition, are meant, are persistent distributers of printed matter, by which they hope to convince, or, at least, to influence their neighbors. Quite recently, such a tract on "Alcoholic Liquids as Therapeutic Agents" was extensively circulated in this vicinity, and doubtless in other places. Were it merely a pamphlet issued by the "Woman's Temperance Publication Association" of Chicago, and distributed hereabouts by the "Boston Women's Christian Temperance Union," any criticism might seem superfluous. But the tract is more than this: it is the reprint of a chapter of a medical work,[1] and, moreover, this chapter is said to have constituted a special lecture, "the closing lecture in the Practitioner's Course for 1884, and was given in compliance with a special request of the class in attendance." As a teacher's statement of the truth about alcohol, as the declaration of an older and prominent physician to younger men just entering the profession, this lecture may very properly attract our attention and criticism, since it is full of error and misstatement concerning the physiological action of alcohol, and the therapeutic inferences drawn therefrom are, to say the least, most doubtful.

It is for this reason that I venture to reopen the question, and to give a brief summary of the chief points of our knowledge of the physiological action of alcohol and alcoholic beverages. And I will say here, for the benefit of any multiscient reader, that while I hope to put together the best and the freshest of our knowledge, I have nothing absolutely *new* to give. While I shall endeavor to make due acknowledgment to the men whose labors have been chiefly valuable in establishing experimentally that portion of our knowledge which has such foundations, it would be as impracticable as it would be tiresome and unprofitable to name every experiment which has been made. Merely to give the enormous literature of the subject, would make a long article.

I. It is generally supposed that alcohol introduced into a sound stomach, in a not too concentrated form, is absorbed *unchanged*. The evidence that a portion is oxidized in the stomach or intestine is not strong; in any case, the amount thus altered must be exceedingly small. If the intestinal tract is affected by catarrhal or other disturbance, the absorption of dilute alcohol will be delayed or prevented, just as that of any liquid would be hindered. On this point, everybody is in substantial accord with the tract. Whether the absorption be altogether by the blood, is open to further investigation. The function of the leucocytes has been so much extended recently, that it would be hasty to deny them a share in the absorption of any substance that passes from the intestine into the body, and Dogiel claims to have demonstrated the presence of alcohol in the thoracic duct very soon after it has been taken into the stomach. The introduction of alcohol into the body appears to stimulate various glands, so that the flow of saliva is increased, and, apparently, that of the gastric juice as well. The immediate effect on any digestion which may be going on has been the subject of much experimental inquiry. The observations which have been made with artificial digestions have generally been unfavorable to any admixture of alcohol, certainly to any considerable one. The experiments of Ogáta[2] on a dog with a fistula also seemed to show that alcoholic beverages cause a delay in the digestive process. The very recent investigations of Gluzinski[3] shed light on the discrepancy hitherto existing between these experimental results and the clinical experience that a sluggish digestion is often favorably affected by a little alcohol. Gluzinski found that, while the presence of alcohol does retard the digestive process (as shown by examination of the substances which can be pumped out of the stomach), the alcohol is rapidly absorbed, and the digestion is then accelerated, on account of an increased secretion of gastric juice. This taking place all the more readily in an empty stomach, would explain the good effects of alcohol in this connection.

II. The alcohol which has been absorbed does not leave the body in any considerable quantity as alcohol, nor as the more immediate products of oxidation (aldehyde or acetic acid). This is, perhaps, the most important truth, and has been most exactly established. It is unfortunate that Dr. Davis has again denied it. He says, page 4 of the tract: "The experimental researches of Lallemand, Perrin, and Duroy proved conclusively that alcohol . . . was eliminated as alcohol, unchanged chemically, from the lungs, skin, and kidneys," and he adds cautiously, a few lines later, that these experiments have been confirmed, "except the claim that the amount eliminated is not equal to the whole quantity taken." Surely, no beginner would infer from the last quotation that *every* competent investigator had found the amount eliminated to be not only "not equal to the whole quantity taken," but really to form only a small fraction of it; yet such is actually the case. This misstatement is aggravated by being followed a little later by another, and perhaps worse one, concerning Anstie's conclusions on the question of elimination.

The researches of the French authors mentioned above[4] have acquired the distinction of serving as the chief weapon of the opponents of alcohol. Their apparent exactness has recommended them to many, and has rendered it difficult to give a complete and convincing demonstration of the falsity of the most important conclusion of the book: "*L'alcool est éliminé de l'organisme en totalité et en nature*" (p. 233). If the reader will take the trouble to examine the experiments on which this conclusion is based, he will be astonished at the weakness of the evidence. If he will also reckon out the amounts of alcohol which it was considered necessary to give to dogs in these investigations, and then state these amounts for a man of average weight, his astonishment will in no wise be diminished.[5] Now it may be very instructive

[1] "Reprinted from a new and very valuable work on 'Principles and Practice of Medicine,' by Prof. Nathan S. Davis. Published by A. C. McClurg & Co., Chicago. A large octavo volume of 900 pages."

[2] Ogáta. Ueber den Einfluss der Genussmittel auf die Magenverdauung. Arch. f. Hygiene, 1885, III. 204.

[3] Gluzinski. Ueber den Einfluss des Alkohols auf die Function des menschlichen Magens, sowohl im physiologischen wie im pathologischen Zustande. Dtsch. Archiv. f. klin. Med., 1886, xxxix., 405.

[4] Lallemand, Perrin, and Duroy. Du rôle de l'alcool et des anesthésiques dans l'organisme, Paris, 1860.

[5] It appears from the memoranda, on pages 47, 57, 63, and 66 of the above work, that an amount of alcohol equivalent to a whole bottle of brandy for an average man was a common dose for the dogs. In

to know just how much alcohol a dog can take and live — or die; to infer the physiological action from these amounts is dangerous; but, as we shall see presently, these investigators, in using such doses, have, in several instances, dug a pit for their own destruction.

When we recall how soluble alcohol is, and the ease with which it is absorbed, it need not seem strange that some should be excreted through the ordinary channels. It does not commonly surprise us that water is excreted, or salt, and we do not venture to build up enormous hypotheses on these facts. No one is ever restrained from using the iodide of potash because of the wonderful speed with which a portion is eliminated, and nowadays, the occasional presence of even a little albumin in the urine is looked upon with great complacency. We ought, then, to be very cautious in drawing hasty conclusions from the partial elimination of alcohol, which undoubtedly often takes place. The question which really concerns us is the amount of alcohol thus disposed of.

I ought to add, in passing, that the work of Subbotin [6] is sometimes quoted, as showing positively a considerable excretion. It has, however, been very properly urged that he only used rabbits in his experiments, and gave them enormous doses of alcohol, so that his results do not really demonstrate its physiological action.

The most careful and extended experiments to determine the amounts of alcohol which pass off through the lungs, the skin, and the kidneys are those undertaken in England by Anstie chiefly, or at his suggestion, and those made by Binz and his pupils in the Pharmacological Laboratory at Bonn. It is, however, worthy of notice, that the work of Lallemand, Perrin, and Duroy, which led to this long controversy, contains its own refutation in itself,[7] and was speedily objected to in its own country (Baudot). The methods for determining the excretion are not all as exact as is desirable. The simplest, (the iodoform test and the test by the reduction of chromic acid) [8] permit the establishment of an upper limit as to the amount of the excretion but do not give an exact quantitative determination. The inflammability or the odor of the products of distillation is obviously the most uncertain of tests as regards quantity. Much more exact would appear to be the determination with the vaporimeter which has been used more extensively at Bonn than in England.

It would require too much space to give the details of the English and German researches. To some extent they may be considered to supplement each other since the latter have developed the quantitative methods more fully, while the careful work of the former has established certain other points very completely, although more indefinitely as to quantity. Anstie [9] and his fellow-workers found that under the most varying conditions and with the use of all sorts of alcoholic beverages, the amount excreted, by dog or man, through all the channels already mentioned, was invariably small. Although Anstie's views are expressed with such perfect clearness in the publications just mentioned, that he who runs may read, Dr. N. S. Davis ventures to twist them as follows (Tract, p. 4); "The late Dr. Anstie, who followed up the investigation of this question with the most commendable perseverance, came to the conclusion that an average-sized adult in ordinary health, was capable of retaining about forty-five grammes (fl. ℥iss) of pure alcohol in the twenty-four hours, admitting that whenever more than this was taken in the time specified, it re-appeared in the evacuations, or was eliminated unchanged." But Anstie's "admission" consisted in the declaration that "the total amount eliminated, however, even in these cases, (that is, for more than ℥iss), was very small." For ℥iij-iv he could never find more than one or two grains in the urine.

Anstie's efforts were ably seconded by Dupré, who in a number of researches showed that the amount of alcohol excreted daily (and which he found to be altogether a minute fraction only of the amount taken), does not increase with the continuance of the alcohol diet, and he showed furthermore, (*contra* Parkes and Wollowicz) that this elimination is practically finished in twenty-four hours after the alcohol is taken.[10] Dupré's experiments have also a peculiar interest because he found that after abstaining from alcohol for ten days he still eliminated a substance through the kidney which when oxidised by chromic acid, yielded a volatile acid having the smell of acetic acid, and therefore simulating the excretion of alcohol as determined by this reaction. But the urine of a teetotaler who had but once in his life taken alcohol, and that two years previously, contained a similar substance at various times!

The work done in reference to this question at Bonn, has yielded quite a literature,[11] and its latest contribution [12] is the result of such careful experimentation under control of the vaporimeter as to leave no possible doubt concerning the amount of alcohol commonly excreted from the body, and I venture to remind the reader that it appeared some months before the delivery of the lecture forming the tract we are considering. I must refer the reader to Bodländer's article for the details of the experiments made on himself and on a

one experiment, the equivalent was as much as two and one-half bottles, and in another case, *three* bottles of brandy.

[6] Subbotin. Ueber die physiologische Bedeutung des Alkohols für den thierischen Organismus. Ztschr. für Biologie, 1871, vii., 361. See Dupré's severe, but just, criticism in the Practitioner, Vol IX, page 28.

[7] The amount recovered, for example, in the urine collected from several men for four hours after the consumption of three bottles of Burgundy and 120 grammes of Cognac, is by a most liberal calculation, less than 1 per cent., (probably not more than 0.4 per cent.) The amount obtained in the respiration experiments, so far as it can be calculated, does not seem to make a much better showing.

[8] Of these the iodoform reaction (which arises on heating dilute alcohol with a solution of iodine in the iodide of potassium decolorized with sodic hydrate) is especially untrustworthy, since carbohydrates, albumin, fibrin, etc., also form iodoform under similar conditions, Although this source of error was made known by Millon, as early as 1845, it was commonly overlooked by later investigators. It has also been found (Lieben) that perfectly normal dog urine contains substances which can be distilled off and give the iodoform reaction. Rajewsky (Pflüger's Archiv., xi. 122), observed that the distillation of perfectly fresh muscles (horse) or liver (horse or dog) or of the brain of a rabbit which had eaten nothing for two days, produced substances which gave this reaction, and, strangest of all, these tissues actually seemed to also contain very small amounts of alcohol. And the test with chromic acid or the bichromate of potash in sulphuric acid, although very delicate, is unfortunately not limited to alcohol. Aldehyde, acetic acid, and many other substances produce the same color change and may be mistaken for alcohol.

[9] The reader will find a sufficient account of this work in Anstie's Stimulants and Narcotics, 1864, and more especially in his "Final experiments on the elimination of alcohol from the body," in the Practitioner, 1874, xiii, 15. The latter article reviews the course of the English researches on the subject, and is particularly interesting because the author died about two months after its publication.

[10] Dupré, "On the Elimination of Alcohol." Proceedings of the Royal Society, 1871, xx, 268. The substance of this paper may also be found in articles in the Practitioner, viii, 148, 224.

[11] Its results are reported in several dissertations and in articles by Binz, who has also briefly collated it in Lectures xxix and xxx, of his recent "Vorlesungen über Pharmakologie." See also Binz, Die Ausscheidung des Weingeistes durch Nieren und Lungen. Arch. f. exp. Path. u. Pharm., vi, 287, and Heubach, Quantitative Bestimmung des Alkohols im Harn, in the same Archiv, viii, 446.

[12] G. Bodländer: Die Ausscheidung aufgenommenen Weingeistes aus dem Körper. Pflüger's Archiv für die ges. Physiologie, 1883, xxxii, 398.

dog. It took 50–100 cm. absolute alcohol diluted with water, equal to two-thirds to four-thirds of a bottle of claret. The dog received rather large doses, 10 to 30 cm. alcohol, or the equivalent of one and one-third to four bottles of claret for an average man. The amounts recovered may be tabulated as follows:

Excreted by	Dog.		Man.		Remarks.
	Experiments	Avge. Recov'd	Experiments	Avge. Recov'd	
Kidneys	4	1.58%	12	1.18%	The chief excretion is during first hour, and is usually reduced to zero after three hours, provided the bladder has been fully emptied.
Skin	2	0	3	0.14	The excretion by the skin is practically over in four hours.
Lungs	3	1.95	3	1.60	The lung excretes alcohol much longer than the other organs, but the amount is small.
Intestines	....	....	1	0	Parkes' determination of alcohol in this connection, was made with fæces which had been preserved several days, so that many fermentative changes may have taken place.
		3.53%		2.92%	

The only other probable path of elimination would be the mammary gland, but experiments with this has led to negative results, and the stories about the intoxication of infants by means of the mother's milk are probably fables.[13]

Bodländer's results for the kidney excretion are in substantial agreement with those of Heubach [14] from the examination of the urine of various fever patients, who had received considerable doses of alcohol (18.0 to 325 ccm.) in different forms. The average output through the kidneys for 22 experiments was 1.12 per cent., or possibly 1.5 per cent., on account of errors in the method.

Even if we allow 10 per cent. for a possible error in the method of Bodländer (and the error of the vaporimeter is not greater than this) we shall find that less than 5 per cent. of the alcohol taken into the body is excreted by the ordinary channels during the entire time that delicate tests show the excretion to continue. In other words, it appears *that at least* 95 *per cent. of any reasonable amount of alcohol introduced into the animal body by the stomach, disappears, and is not found in the excretions, either as alcohol or as its more immediate oxidation products* (*aldehyde or acetic acid*). This is result not merely of Bodländer's work but of Anstie's and of the experiments of many others. All experimental results of a contrary nature are either inexact in methed or have been arrived at by the use of quantities of alcohol so large as to be considered unreasonable.

III. The alcohol which is taken up into the body and not excreted disappears; it is transformed in some way not made out as yet, and *is not stored up as alcohol.*

[13] See Bodländer, op. cit., p. 425.

[14] See also Heubach, Ueber die Ausscheidung des Weingeistes durch den Harn bei Fiebernden. Inaug-Diss. Bonn, 1875.

This is evident from a number of considerations which have an experimental basis. In the first place, the determinations of Anstie and Dupré showed that the excretion does not increase when the alcohol diet is continued, as it might be expected to do if the alcohol remained in the body as alcohol. Should it be urged that this excretion is already the greatest possible, it would be well to consider an experiment made by Anstie [15] in 1873. He gave a small dog (9¾ pounds) one ounce of brandy (containing 198.9 grains of alcohol) daily for a number of days. The excretion on the tenth day was only 1.13 grains of alcohol. On the eleventh day, two hours after receiving half-an-ounce of brandy, the dog was killed, and very rapidly cut up into very small pieces, and every fragment of the animal was put into water for extraction. A few hours later this mixture was examined, and only twenty-four grains of alcohol could be recovered, or not more than one-fourth of the amount which had been taken but a couple of hours before. Remembering that the elimination of the previous day was only 1.13 grains, we must infer that the dog had used up some eleven ounces of alcohol with great completeness. This corroborates an earlier discovery made by Schulinus and Sulzynski, pupils of Buchneim.[16] In the course of some experiments showing that little alcohol is excreted, and that the blood contains, relatively, rather more than other tissues, it was found that fresh blood causes a portion of the alcohol which has been added to it to disappear. The loss seems to be, at least, ten per cent. This is in some way a "vital" phenomenon of the blood, for blood which has been carefully kept from eighteen to twenty-four hours disposes of much less alcohol, apparently less than one per cent. Lallemand, Perrin, and Duroy [17] held that the alcohol accumulated in the blood, but especially in the brain and liver. Their figures for the amount in the liver permit no convincing calculation of the proportion of the alcohol to be found in that organ. As to the brain and the blood, such statements as are so made as to permit a calculation, show that, although very large doses were given, not more than one per cent. of the alcohol was found in the brain (experiments with six dogs), and not more than eleven per cent. in the blood (experiments with two dogs). A still further incidental corroboration of the disappearance of alcohol I shall refer to later.

I do not see how the conclusion can be avoided that alcohol is disposed of as really and as truly as any food. In fact, if we could consider its behavior as thus far presented without any bias whatever, I doubt if any one would hesitate to call alcohol a food.

I venture to place here the almost pathetic words with which Anstie closed the report of his "Final Experiments," which were, unfortunately, to be really final for him:

"I therefore trust that we may consider one important portion of the alcohol question to be closed. It is certainly rather hard that the very inadequate researches of Lallemand, Duroy, and Perrin should have been allowed so long to mislead the majority of the profession and of the public upon the subject of the elimination of alcohol, being, as they were, mere qualitative experiments, and, even as such, devised and

[15] Anstie. Final experiments, etc.

[16] Schulinus. Untersuchungen über die Vertheilung des Weingeistes im thierischen Organismus. Arch. d. Heilkunde, 1866, vii., 97. See, also, Harnack's edition of Buchheim's Lehrbuch der Arzneimittellehre, 1883, p. 558.

[17] Du rôle de l'alcool, pp. 63, 82, 230, 233.

carried out with such an absence of all reasonable precaution against fallacy, as should have set physiologists on their guard at once. As it is, it has cost some fourteen years of almost unintermittent work to explode the errors which the French observers made current respecting a merely preliminary investigation into the action of alcohol. I appeal to the respectable members of the teetotal party, and I put it to their sense of honor not to continue to circulate the gross misstatements on this subject which, even now, are circulated broadcast in the tracts with which their society floods the country. It cannot do the temperance cause any good in the end; indeed, the discovery that they have been systematically misled on a point to which their informers could have no difficulty in ascertaining the truth, has already produced a strong revulsion in the minds of many persons against everything that bears the most distant relation to teetotalism."

Nearly another fourteen years have passed since these words were written, and yet the same misstatement continues.

IV. Various practical considerations have led physiologists, as well as pharmacologists, to examine the influence of alcohol, particularly on the body temperature, on the heart, and on the transformation and tissue changes in the body, which are now very commonly called its metabolism.

Historically, it seems to have been chiefly the favorable results obtained by the use of large doses of alcohol in the treatment of fevers, especially in England, and there particularly under the leadership of Todd, which directed attention to the question of temperature and eventually to the other points of inquiry. The well-known "warming" effect of alcoholic drinks which manifests itself at first in the epigastric region, "making glad the heart of man" seemed at variance with its favorable action on fevers. It was thought by theorists that alcohol ought to raise the temperature and so be harmful in fever, but careful investigation has shown that alcohol tends to lower the temperature of the body. Dr. Davis (page 3 of our tract) suggests that this knowledge is due to him, having been demonstrated in experiments which he made in 1850, and used in a paper[18] read before the American Medical Association in May, 1851. As there detailed his experiments prove nothing of the kind. He mentions *two* experiments made apparently with no control experiments whatever, and showing a fall of 0.5° F., which is attributed to the brandy taken. When we remember that these observations were made between nine and eleven o'clock at night, in other words, at a time when the temperature would naturally sink, we shall see how little credit is to be attached to them. Not only were Dr. Davis's observations quite inconclusive, but they are not even the earliest. A couple of years before, Duméril and Demarquay[19] had demonstrated that alcohol causes a very considerable lowering of the temperature. Their method was not altogether unobjectionable; the control experiments do not appear to have been sufficiently numerous, and the doses were very large. Still earlier Nasse,[20] had observed a fall of temperature in rabbits, but he laid no stress upon it.

Here, again, the Bonn pharmacological laboratory has been active in making the measurements required to clear up this question.[21] The fact is, that it is not always quite as easy to affect the temperature by alcohol as many who are but partially familiar with the subject commonly suppose. All careful investigators are agreed that alcohol taken into a healthy animal body never raises the temperature. As to the lowering, the following statement, condensed from Binz (Vorlesungen, page 360), gives the view which has a good claim to general acceptance: The temperature of healthy persons is not sensibly altered by small amounts of alcohol, which, however, may be large enough (particularly in a concentrated form) to produce a feeling of warmth in the stomach and later in the skin. Moderate amounts of alcohol, 30 to 80 grammes, (that is, two-fifths to one bottle of claret), may cause the temperature to fall 0.3° to 0.6° C. (0.5° to 1.1° F.)and this without the production of intoxication. This reduction of the temperature is also evident at the time of day when a rise would be naturally expected, (Daub), but the amount of the reduction is very much lessened by the habit of using alcohol. Narcotic doses, on the other hand, reduce the temperature several degrees and this continues for some hours. In other words, a glass of whiskey or a pint of beer has no appreciable effect on the body temperature; doubling the dose will perhaps cause the temperature to fall somewhat in those unaccustomed to the practice; complete intoxication (from alcohol) is accompanied by a great fall of temperature.[22]

The conclusions of Ringer and Rickards[23] and those arrived at by Parkes[24] are in substantial agreement

[18] An Experimental Inquiry concerning some points in the Vital Processes of Assimilation and Nutrition, Northwestern Medical and Surgical Journal, 1852, iv., 169.

[19] Duméril et Demarquay. Recherches expérimentales sur les modifications imprimées à la température animale par l'éther et par le chloroforme, et sur le mode d'action de ces deux agents. Arch. gén. de méd. 1848, xvi., 334.

[20] Nasse. Ueber die Wirkung des Aderlasses auf die thierische Wärme. Med. Correspondenzblatt rhein. u. westf. Aerzte 1845, iv., 346.

[21] See: Binz, Sitzungsber d. Niederrh. Ges. 1869; Berl. kl. Wochenschr, 1869, 334.
Bouvier: Untersuchungen über die Wirkung des Alkohols auf die Körpertemperatur. Pflüger's Arch., 1869, III, 370.
——: Alkoholstudien. Ctbl. d. med. Wiss. 1871, 801.
Daub: Ueber d. Wirkung des Weingeistes auf d. Körpertemperatur. Arch. f. exp. Path. u. Pharm., 1875, III, 260.
The literature is very fully given in Binz's "Vorlesungen" including several dissertations which I omit.

[22] And this fall is all the greater when the drunkenness happens to coincide with exposure to cold. Several illustrations of this have become classic:
See: Magnan, Gaz. méd de Paris, 1870, 88: "Une femme ivre, qui était restée pendant plusieurs heures exposée à un froid très-vif, a présenté au rectum et à l'aisselle une température de 26°, (78.8° F.;) qui s'est élevée progressivement, de huit heures du matin à quartre heures du soir, jusqu' à 37°, température normale. La malade, du reste, est sortie guérie de l'hôpital."
Reincke, Beobachtungen über d. Körpertemperatur Betrunkener. Dtsch. Arch. f. kl. Med., 1875, xvi, 12. These were cases brought in by the police. Temperatures from 30° to 35.4° C. (86° to 95.7° F.), were common. In some lethal cases it was still lower. His most extraordinary temperature (p. 15), was in a man who was found drunk shortly after midnight. He was transferred to the hospital in the morning with a rectal temperature at 8 o'clock of 24° C. (75.2° F.) There was gradual improvement during the day and at 7 o'clock the next morning the temperature was normal and he was discharged at noon.
Weckerling, same Arch., 1877, xix. 317. Rectal temperature of 35.4° C., (95.7° F.) in a child (three and three-quarters years), which had drunk a large quantity of "schnaps."

[23] Ringer and Rickards. Influence of Alcohol on the Temperature of Non-febrile and Febrile Persons. 1866. Proc. Roy. Med. and Chir. Soc., v. 209.

[24] See: Lond. Med. Rec., 11 Mch. 1874. Parkes' chief contributions which bear on this point are:
(1) Parkes and Wollowicz. Experiments on the Effect of Alcohol (Ethyl Alcohol) on the Human Body. Proc. Roy. Soc., 1870, xviii, 362. (No unquestionable reduction from 1-8 oz. of rectified spirits daily, or from 1-2 bottle of fine brandy in divided doses. A slight rise of 0.5° F., is complicated by the presence of a "febrile catarrh.")
(2) Parkes and Wollowicz. Experiments on the action of Red Bordeaux Wine (Claret) on the Human Body. Proc. Roy. Soc., 1871, xix, 73. (Practically no effect from 10, or from 20 oz. of good claret; certainly no rise of temperature.)
(3) Parkes. Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen and on the Pulse and Temperature of the Body. Proc. Roy. Soc., 1872, xx, 402. (No depression of temperature for three daily doses of brandy of 4 oz. each.)
(4) Parkes. On the Influence of Brandy on the Bodily Temperature, the Pulse, and the Respirations of Healthy Men. Proc. Roy.

with the views I have noted from Binz. Parkes seems to have found that the small reductions of the temperture are brought about by smaller doses, while the body is at rest and especially while fasting, than with food and exercise. In general, his men (soldiers) required rather large doses to affect the temperature, but they were all accustomed to alcohol. Or is it possible, as an eminent physiologist once suggested privately to me, that the temperature of the body has a somewhat different regulation in England from that on the Continent?

The reduction of fever temperatures by the use of alcohol has been equally fully demonstrated, and is generally admitted, so that alcohol is also considered by many that have seen this effect, to be a very important antipyretic. It would, however, lead much too far, were we to examine this aspect of the question, and I only wish to consider the physiology of the subject.

We come now to the consideration of an effect where the experimental evidence is in a very unsatisfactory condition — that on the heart. It is a fact often enough observed clinically that a weak heart can be quickened and strengthened by alcohol administered either hypodermically or through the intestinal tract. It is, however, somewhat uncertain whether we have to do here with a direct action on the heart or with a reflex stimulation.[25] On the other hand there is some evidence that such an effect is altogether problematical. Zimmerberg's dissertation (written at the suggestion of Schmiedeberg in 1869) is commonly given as authority for the statement, the stimulating influence on the heart does not exist. It has however, been urged the Zimmerberg's doses were toxic.[26]

Martin, too, in an article which I shall quote presently, seems to have found that a small dose of alcohol well diluted (15 cc. in 50 of water) does not quicken the heart-beat of a teetotaler.

The direct action of alcohol on the mammalian heart has only recently been studied, in fact the Baltimore method of isolating the dog's heart so as to permit such investigations is itself quite new. By ingeniously picking out what he wanted, Dr. Davis derives great comfort from the work of Martin and Stevens.[27] These investigators found that defibrinated blood fed directly to the heart (only the heart and lungs being in the circulation in this method of working) and containing 1-2 per cent. by volume of ethyl alcohol, invariably caused a very rapid and marked diminution in the work done by the heart in a given time as indicated by the amount of blood pumped out. With blood containing 1-4 per cent. alcohol the effect is in most cases the same, but sometimes little or none, and no influence is exerted by blood with but 1-8 per cent. at least for several minutes. This effect is due to a distension of the heart, the systole growing less and less complete. Martin calculates the amount of alcohol which would be 1-4 per cent. of the blood of an adult man and calls it about 15 ccm., or the equivalent of a good "glass" of brandy. Thus far Dr. Davis quotes him correctly in his lecture, but appears to have overlooked the following very important passage with which Martin's article in the *Maryland Medical Journal* ends (p. 294):

"We have made a few experiments to see what dose of alcohol given by the stomach to a dog will produce some similar action on the heart. When the heart lies in the body and in connection with the central nervous system there are of course considerable difficulties to be overcome, and all we can say as yet is that to get any distinct influence on blood-pressure one must put much more alcohol into the stomach than an amount equal to 1-4 per cent. of the total blood of the animal. It is either not absorbed fast enough to reach at any moment the heart-poisoning limit, or, more probably, is picked up by other organs, very likely the liver, and held back from the heart.

"We then tried in another way, by directly injecting into the jugular vein of a curarized dog, a small quantity of salt solution containing an amount of alcohol equal to 1-4 per cent. of the total blood of the animal reckoned as 1-13 of the weight. In such cases we found usually a very temporary enfeeblement of the heart, indicated by a lower arterial pressure, but this seems only to last while the injected solution is flowing through the organ, or for a few seconds afterwards. Before the blood returns it has apparently deposited its alcohol elsewhere in the body, or at any rate got rid of it somehow, so that it no longer acts immediately upon the heart, at least to a directly noticeable extent."

It will thus be seen that although the results of Martin and Stevens are probably true for alcoholized blood circulating through the heart, the body does not permit the heart to be reached ordinarily by any such amount of alcohol. The reader will see in this fresh evidence that the body disposes of the alcohol by some other process than excretion. This behavior of alcohol need not particularly astonish us, for we know now that various products of the digestive process ("peptones" or diffusible bodies standing near these), which in all probability are readily taken up from the intestine, cause much disturbance when injected directly into the blood. There is, however, some experimental evidence that alcohol really stimulates the heart and raises the blood-pressure.[28] The investigations of Parkes and Wollowicz, and those of Parkes alone, certainly show a considerable quickening of the

Soc., 1874, xxii, 172. (The Temperature of a man remaining quietly in bed was not especially changed by brandy (1 to 6), taken four hours after breakfast. During complete inanition (no food from 6 P. M., until 1 P. M., 6 ozs. of brandy being taken at 10 A. M.), the temperature was more distinctly lowered, but the average for the three hours is only 0.6° F.)

[25] Bleuler and Lehmann have recently shown (Archiv. f. Hygiene III. 215) that the pulse is readily quickened by all substances (hot water, salt solutions, etc.) which cause any feeling of burning, pressure or nausea in the stomach, or a fulness in the intestines, especially in the rectum.

[26] Zimmerberg, Untersuchungen über den Einfluss des Alcohols auf die Thätigkeit des Herzens. Dorpat, 1869. An examination of this dissertation certainly does show very large doses for frogs, rabbits, and cats, but it cannot be urged that those given the men were unduly large. The average pulse, as I figure it from the data printed, really shows less increase from alcohol than we should expect from clinical and other experience, and the blood-pressure (cats) is undoubtedly lowered in a way which does not agree with the results of other observers. The rabbits showed a very marked rise of the pulse-rate, and Zimmerberg endeavors to explain this by an experiment with the injection of water (into the stomach) which was also followed by increased heart activity. But the water effect only lasts forty minutes, while the alcohol quickening still continued at the end of the experiments, or some sixty-five minutes after taking the alcohol. Of all the animals used for this work the rabbit is the most likely to have had a considerable quantity of food in the stomach. It is, therefore, a plausible explanation that the slowness of the absorption and the continuance of a mild reflex stimulus may have raised the pulse, and it is also noticeable that the dogs, which apparently received more moderate doses do show some quickening of the pulse shortly after the alcohol was given.

[27] Martin. The direct action of ethyl alcohol upon the heart. From the Transactions of the Med. and Chir. Fac. of Maryland, 1883. In Maryland Medical Journal, September 8, 1883, p. 289. Experimental details of these experiments are given in the "Studies from the Biological Laboratory," of Johns Hopkins University, Vol. II, p. 477.

[28] See Dogiel in Pflüger's Archiv viii. 605, (unfortunately only a *résumé*) and the *Referat* on the (Strassburg) dissertation of Rioschiro Maki in the Centralblatt f. klin. Medicin, 1884, 685. The latter's experiments appear to have been made on the frog's heart with Williams' apparatus, and are in a measure opposed to the conclusions which Davis draws rather loosely, as usual, from the work of Ringer and Sainsbury. (Practitioner, 1883, xxx, p. 339.)

pulse in the soldiers they experimented on. This quickening seems to last too long to be merely the result of intestinal stimulation. Their sphygmograms will apparently bear the interpretation that the tension is increased, but all sphygmographic work is more or less enigmatical.

Here belong, too, the apparently careful experiments of Castillo,[29] who found (for rabbits and dogs) that small doses of alcohol accelerate the pulse by direct stimulation of the heart (the nerves being cut) with an increase of the cardiac force and a higher arterial pressure, while larger doses accelerate the pulse but diminish cardiac force and lower the arterial pressure. With an excessive dose the pulse-rate is diminished from the first; there is a direct paralysis of the heart, which always comes to a standstill in diastole. This shows, what is also generally admitted, that there is a period of the alcohol effect, often easily and rapidly reached (it being probably rather a question of quantity rather than of time), when the heart is weakened, the bloodvessels much relaxed and the blood-pressure greatly lowered. This is probably a nervous phenomenon and due to a paralysis of vasomotor centres, for Kobert found that bloodvessels showed no special change when alcoholized blood circulated through them.[30]

(To be continued.)

THE SURGERY OF THE ABDOMEN ILLUSTRATED BY EIGHT CASES.

BY D. W. CHEEVER, M.D.,
Professor of Surgery, Harvard Medical School; Visiting Surgeon, Boston City Hospital.

CASE I. PERICŒCAL ABSCESS.[1]

THE patient was a girl of ten years, who, though always delicate, had never up to the present illness, had any serious disease. For about six weeks before she was seen by me in consultation, she had suffered from abdominal pain with occasional vomiting, and feverishness. On examination there was found to be a solidification of a large part of the right lung which interfered seriously with respiration. Indistinct fluctuation was made out in the right iliac region and by aspiration at a point midway between the umbilicus and the crest of the ilium, about sixteen ounces of pus was drawn out. As at this time the child was much exhausted and the removal of the pus afforded relief, it was thought wiser to pause here. The abscess, however, refilled, and eighteen days after, the aspiration was laid open.

It was found necessary to make the abdominal incisions, one in the median line, and one at each side of it. A large quantity of foul pus was let out. The finger introduced through the median incision entered at once a large abscess cavity, extending across the abdomen from side to side, which must have held at least a pint and a half of pus. No connection with the viscera was detected, and the intestines were apparently walled off by inflammatory exudation. The cavity was washed out with carbolic solution, large drainage-tubes were inserted into the two lateral incisions and a braid of silk into the median one. Carbolized oil dressings were used. After the operation there were no signs of collapse, and the temperature fell rapidly to normal. The cavity was daily washed out with a solution of corrosive sublimate. The patient began to gain flesh and strength, and three weeks after the operation the middle sinus closed, the silk having been removed in the mean time.

Six weeks after the operation the sinus on the right side had closed permanently. That on the left nearly healed up, but began finally to discharge fæcal matter. Two months after the operation this fæcal fistula healed spontaneously.

The right lung did not entirely clear up, until about four months after the operation, at which time the child was well and strong.

The final formation of a fæcal fistula seemed to indicate some low or tubercular ulceration of the bowel. The original site of the abscess was near the cœcum and ascending colon. There was no stoppage of the bowels. Considering the feeble constitution of the child and the infarction of the right lung, together with the long duration of the illness, the recovery was a remarkable one. The spontaneous closure of the fæcal fistula was also remarkable, although this is often seen after hernia and strangulation.

CASE II. PERITYPHLETIC ABSCESS.

The patient was a Swede, twenty-three years old. He was undersized and poorly nourished. On account of the language it was impossible to obtain a complete history. So far as could be made out, however, the patient received a blow in the left groin about three months before he first came under my care, and for the last month or so pain had been present in the abdomen, gradually increasing in severity. This pain has been so constant and severe as to prevent sleep. The patient had lost flesh and strength and had little or no appetite. Micturition difficult and painful. The abdomen was enlarged symmetrically. The catheter drew off twenty ounces of clear urine, after which the right lower abdomen seemed somewhat more prominent than the left. The aspirating needle was inserted in the median line four inches below the umbilicus, but nothing was found. There was a fluctuating area on the left of median line, and the aspirating needle was again inserted, this time an inch below and to the left of the umbilicus. A few drops of pus were drawn out. With the needle as a guide an incision was made down to the peritoneum, but no pus was found. The finger introduced into the peritoneal cavity detected an induration below and to the right, which could be grasped between the finger of one hand within, and the other hand without. The needle was then introduced at a point two inches to the right, and three inches below the umbilicus, and considerable thick, foul pus evacuated. Using the needle as a director a free opening was made into the abscess cavity. The index finger introduced into this cavity, discovered three apparently blind pouches; one directed upwards and towards the first incision, a second passing outwards and towards the flank, and a third passing backwards. The abscess-cavity was irrigated with carbolic solution, and drainage-tubes were introduced. The first incision was carefully sutured, and sealed with the compound tincture of benzoin. A rectal examination was now made, and a fluctuating projection discovered on the

[1] This case occurred in the practice of Dr. G. E. Titcomb, of Concord, Mass.

[29] Castillo. The physiological action of alcohol on the circulation. Philadelphia Medical Times, 1880, xi, 44. This is an abstract of a prize inaugural essay, and unfortunately does not contain full details.
[30] Cf. Binz' Vorlesungen, p. 386. Kobert. Ueber die Beeinflussung der peripheren Gefässe durch pharmakologische Agentien. Arch. f. exp. Path. u. Pharm., 1886, xxii, 77.

Original Articles.

ALCOHOL AGAIN: A CONSIDERATION OF RECENT MISSTATEMENTS OF ITS PHYSIOLOGICAL ACTION.[1]

BY JOSEPH W. WARREN, M.D.,
Assistant in Physiology in the Medical School of Harvard University.

A substance which is disposed of as completely as is alcohol by the human body, might be expected to exercise some influence on the nutrition. A large number of cases are cited by Anstie (in his "Stimulants and Narcotics") all showing that a human body well supplied with alcohol may get along very well or fairly well without the ordinary quantity of food. The clinical experience with alcohol, under conditions where it could not merely act by aiding the patient to absorb more food, also suggests that it may, at least at times and to some extent, take the place of acknowledged foods. In much of the discussion on this point, it seems but too often overlooked that alcohol, as a non-nitrogenous substance, could not be expected ordinarily to actually take the place of all and every food for a long time. It does not surprise us that men do not get on very well when fed on fat or sugar only, why should we expect them necessarily to thrive on alcohol? It is, however, a matter of common experience that people do thrive and keep perfectly well who use a moderate amount of alcohol in the form of various beverages. Some of these contain other substances of unquestioned nutritive value. The fact must not be overlooked that there are millions of people in this country and millions in Europe who constantly take small quantities of alcohol in some form and who are absolutely in no way the worse for so doing. It will not do to point to the drunkards, whether of the fat and lazy type or not; it will not do to urge that organs degenerate as a result of alcoholic excesses; these things merely have a value in enabling us to draw some line as to what is reasonable and prudent, but they do not demonstrate the injuriousness of the substance properly used. It is easy to find instances where acknowledged foods are abused. All about us are men who undoubtedly eat too much, or do too little, or both. There is good reason to believe that many persons drink too much water, while the abuse of coffee and tea is very frequently observed. It would be easy to multiply the instances where the good things of life, which are also its necessities, are abused, but no one ventures to infer that these things are therefore harmful. The degenerations of phosphorus poisoning are never advanced as an objection to the acid phosphates of Tom, Dick, and Harry, nor as showing the harmfulness of such phosphorus acid as is in accepted and traditional foods.

The experimental determination of the position of alcohol as a food, has hitherto met with great difficulties. Its disappearance suggests that it *may* play a part in the economy of the body in one of several ways. If it be burned up in the body, that is, completely oxidised to carbonic acid and water, as seems highly probable, we should not only have the development of a great amount of force, but we should also expect to find the products of this combustion, and these have not as yet been satisfactorily made out. There are, however, a number of good reasons for this, and it by no means follows that they do not exist.[31]

As there is no nitrogen in alcohol itself, we may not expect any direct effect from the oxidation of this substance on the nitrogenous excretion. Indirectly, the influence might be either to increase or to diminish it. Alcohol might work unfavorably on the metabolism, causing more extensive tissue change and consequently an augmentation of the nitrogen output; or it might for some reason, while increasing the destruction, hinder the outflow, but this view is exceedingly improbable, and has no good foundation. On the other hand, the combustion of the alcohol may take the place of the oxidation of the nitrogenous substances (just as it may be easier at times to burn the wood a household happens to have at hand and spare the supply of coal); this would lead to a diminished excretion of nitrogen. Simple as the problem appears, the solution has not yet been reached. A large number of observers report a distinct diminution of the nitrogen output. Even Dr. Davis admits this, but he interprets it ingeniously as a retardation of "those molecular or atomic changes which constitute nutrition, disintegration and secretion, and on which the phenomena of life depend," and also as a retardation of the "elimination of waste matter." This, in very plain English, is much the same as saying that a family which happens to use a gas stove for a few days, ought to have as full an ash barrel as when using the ordinary range, otherwise they fail to eliminate the waste matter of the coals which they have not as yet burned! Although personally, I am inclined to think that there really is a diminution of the nitrogen outgo, I do not think that the evidence at our disposal fully proves it. None of the older experiments have much value now. We have learned, within a comparatively few years that to understand the nitrogenous metabolism the body experimented upon must be brought into nitrogenous equilibrium before any drug or other influence can be tested. Before studying a drug we must have been able to find all the nitrogen that goes into the body in the urine and in the fæces, otherwise no fair inference can be drawn, because the body may be adding to its own albuminous material. And when we have this equilibrium we must still know not merely the nitrogen of the urine but also that of the fæces. Evidently it is not enough to ensure a regular and measured diet, from which an unvarying import of nitrogen is inferred. All this was unknown to the older experimenters, and very much limits the value of their work, however excellent it was in its own day. It has also often not been duly considered in much work of a more recent date.

It is for these reasons, (and I think they are not finical), that we are not justified in accepting as conclusive, many experiments commonly referred to as showing a reduction of the metabolism with reference to nitrogen, under the influence of alcohol, that is, a saving and storing up of albuminous material. To these belong the elaborate work of Böcker[32] (who

[1] Concluded from page 6.

[31] Thompson (Is Alcohol a Food? Lancet, 1885, I, 743), has suggested an ingenious plan to determine whether alcohol can act as a food. He argues that alcohol ought to appear in the urine more readily when the body is fully supplied with food than when there is a deficiency (as with little food and much exercise), which it may make up. A couple of experiments seem to corroborate this view, but evidently there are too many sources of error in the method there employed.

[32] Böcker: Beiträge zur Heilkunde, 1849, vol. I, 240. It is, of course, easy to sneer, now, at work of this description which in its day and generation was unquestionably on a high level. I have no desire to speak of such investigations otherwise than with respect. What ought to meet with vigorous opposition is the fact that a

seems to demonstrate a great falling off in the elimination of nitrogen, but with absolutely no control over the amount of nitrogen entering the body), that of Duchek, the investigations of Hammond[33] (with an apparent lessening of the nitrogenous excretion in the urine of about thirteen per cent.), those in the first three articles by Parkes noted above, (with practically no change in the urea outflow), and the more recent researches of Riess[34] (a lessening of fifteen to twenty-two per cent.).

In fact, almost the only experiments in which the nitrogenous equilibrium seems to have been carefully established are those of I. Munk[35] on dogs. Some earlier ones made by Fokker, and which appear to have been good (showing a diminution of from six to twenty per cent.) are, unfortunately, not easily accessible to me now. Munk found that moderate doses of alcohol diminished the nitrogen of the urine from six to seven per cent., while larger doses (not lethal, but producing much depression and insensibility) caused an increase in the nitrogenous output of from four to ten per cent. An independent confirmation of these results would be very desirable. It may be that alcohol can protect the consumption of other substances to some extent, but that large amounts either injure living tissues, or cause a destruction of albuminous material not yet worked into the living protoplasmatic network of the body.[36]

The metabolism of the body is not measured by the excretion of nitrogen alone, but also by the amount of carbonic acid sent out. Many writers are disposed, nowadays, to attribute the latter to the oxidation of fat and carbo-hydrates, and to speak of the former as due to the transformation of albuminous substances. Such a view I believe to be exceedingly incomplete and unsatisfactory, but it would lead too far to consider that question now. Whatever the origin of the substances may be, there is no question that their formation goes on in such a way that the carbonic acid may be considered to be, in general, quite independent of the urea.

At the first glance, it would appear to be simple enough to determine this carbonic acid excretion, and to draw inferences from it. Indeed, there are numerous older observations where such a course was pursued. It has turned out, however, to be of prime importance to know the amount of oxygen taken into the body, as well as the amount of carbonic dioxide excreted.[37]

A body like alcohol, burning readily and completely to carbonic acid and water, might be expected to demonstrate its combustion by affecting the CO_2 excretion, and many observers have sought to determine this experimentally. For various reasons, no satisfactory unanimity of results has as yet been attained. The older observations are often inexact in method, although they generally show a diminution in the CO_2 output. To these belong the experiments of Prout,[38] Fyfe,[39] Vierordt,[40] Böcker, Davis, Hammond, and others, in which there was absolutely no determination of the oxygen, and the collection of the carbonic acid was made in such a way, or for such periods, as seem too little exact and sufficient. The ingenious and elaborate researches of Smith[41] also fail to give the information we want concerning the amount of oxygen taken in. For the carbonic acid excretion, it was found that it is increased by spirits of wine and most beverages containing alcohol, but diminished by brandy, gin, and some specimens of whiskey.

Berg,[42] who also found the absolute amount of carbonic acid increased after alcohol, does not appear to have made any determination of the oxygen.

Among the more recent investigators with exacter methods, Boeck and Bauer[43] found that alcohol in small doses diminished the O_2-inflow and the CO_2-excretion (about 20 per cent.) while larger quantities (without narcosis) caused an increase as regards both substances of 12–34 per cent. Only two experiments (dogs) are reported, but the authors say they obtained similar results on other dogs and cats. In some cases there appears to have been distinct intestinal disturbance, and this would probably affect the CO_2 production.[44] In one of the two cases the increase of oxida-

teacher should cite such experimental work of an older date as valuable, and yet draw no distinction between it and that done in recent years with the most carefully elaborated methods, or even neglect to mention the latter altogether.

[33] Hammond: The physiological effects of alcohol and tobacco upon the human system. Reprint from the American Journal of Medical Sciences for 1856, in his Physiological Memoirs, 1863, p. 43. Hammond's urea determinations have not met with universal acceptance. Cf. Speck: Arch. f. exp. Path. u. Pharm., xv, 95.

[34] Riess: Ueber den Einfluss des Alkohols auf den Stoffwechsel des Menschen. Zeitschr. f. klin. Med., 1881, vol. II p. 1. Binz (Vorlesungen p. 361, and again quite recently in the Centralblatt für klin. Med. for May 7, 1887,) seems to consider Riess's results satisfactory. I must observe, however, that the men examined were convalescents (from rheumatism and subacute myelitis), that although they were placed upon a carefully measured diet the nitrogen of this was not determined, and that the men were admittedly not in nitrogenous equilibrium.

[35] I. Munk. Ueber d. Einfluss des Alkohols u. des Eisens auf den Eiweisszerfall. Verhandl. d. Berliner physiol. Ges., 3 January, 1879. Archiv f. (Anat. u.) Physiologie, 1879, 163. See, also, Munk-Uffelmann. Die Ernährung des gesunden und kranken Menschen, 1887, p. 60.

[36] The presumption that alcohol lessens the amount of oxidation in the body, in the sense of saving material, not of retaining "effete, waste substances," is strengthened by some interesting observations on the conversion of benzol to phenol and related products in the body. Nencki and Sieber (*a*) had shown that, normally, a pretty constant relation existed between the amount of benzol (C_6H_6) introduced into the body and the phenol (C_6H_5·OH) found in the urine. Then Simanowsky and Schoumoff (*b*) found that alcohol lessened this transformation for rabbits, dogs, and for man.

(*a*) Nencki and Sieber. Ueber eine neue Methode die physiologische Oxydation zu messen und über den Einfluss der Gifte und Krankheiten auf dieselbe. Pflüger's Archiv, 1883, xxxi, 319.

(*b*) Simanowsky and Schoumoff. Ueber den Einfluss des Alcohols und des Morphiums auf die physiologische Oxydation. Pflüger's Archiv, 1884, xxxiii, 251.

[37] The relation of these, $\frac{CO_2}{O_2}$, is now commonly called the respiratory quotient; it expresses the amount of oxygen returned as carbonic acid, any deficit being used presumably for other oxidations.

[38] It is an entertaining illustration of the danger that lurks in citations, that Dr. Davis speaks of Prout as overthrowing, "by direct experiment," the theory of Liebig after it was proposed, Prout, as a matter of fact, having made his investigations when Leibig was not yet ten years old. Dr. Davis (in common with many others, for example, Wolters) has been misled by a quotation in the second volume of the London Lancet (1843, p. 17). Prout's articles — "Observations on the Quantity of Carbonic Acid Gas emitted from the Lungs during Respiration, at different Times, and under different Circumstances" — are in Thomson's "Annals of Philosophy," 1813-14, Vol. II, p. 328, and Vol. IV, p. 331. Interesting as his work is, it has, of course, now only historical value. Any deductions which modern writers may wish to make from it must be modified also by Prout's statement (p. 336 of the second article) "that they (the numbers in his paper) do not represent the measures of the quantity of carbonic acid emitted *in* any given time from the lungs, but the measures of the power or capability of the lungs, *at* any given time, to form or throw off carbonic acid. What I mean to say is this: that the *power* or *capability* of the lungs for forming and throwing off carbonic acid is *greater* at noon, etc., and not that a greater quantity of it *is actually thrown off* at that time than at any other. A greater quantity will, indeed, be thrown off, cæteris paribus, on an equal number of similar respirations being made in the same given time; but whether this be really the case or not, I cannot pretend to determine."

[39] Fyfe in an Edinburgh dissertation, 1814, quoted by Prout.

[40] See Wagner's Handwörterbuch d. Physiologie, II, 884.

[41] E. Smith. Experiments of the action of food upon the respiration. Brit. Med. Journal, 1859, 254. Practical deductions from an experimental inquiry into the influence of food. Ibid, p. 433. His paper in the Proc. Roy. Soc., 1859, p. 638, is only an abstract.

[42] Berg. Ueber den Einfluss der Zahl und Tiefe der Athembewegungen auf die Ausscheidung der Kohlensäure durch die Lungen. Dtsch. Arch. f. klin. Med., 1869, vi, 291.

[43] H. v. Boeck and J. Bauer. Ueber den Einfluss einiger Arzneimittel auf den Gasaustausch bei Thieren. Zeitsch. f. Biol. 1874, x, 336.

[44] Zuntz and v. Mering (Pflüger Archiv, 1883, xxxii, 173) have shown that the work of the digestive act (and apparently any irritation of the tract) causes an increase in the amount of oxygen taken into the body.

tion is not convincing, for a change nearly as great occurs apart from any alcoholic influence.

In 1883 Henrijean published a preliminary communication [45] concerning experiments made on himself in Fredericq's laboratory at Liége, in which he found that the oxygen taken into the body was, on an average 19 per cent. greater in amount after taking alcohol than in hunger, and the increase after taking a moderate breakfast was about 23 per cent. The increase under alcohol seems to continue too long to be due to the action on the intestines; in fact the average for the later determinations (80–180 minutes after taking) is rather higher than for the earlier ones. No determination of the carbonic acid was made.

At about the same time Wolfers [46] published some experiments which he had made under Zuntz's guidance a couple of years before. He used rabbits which were sunk in a bath at a constant temperature. The alcohol was introduced sometimes through a vein, in some experiments directly into the stomach. The doses were large although narcosis does not seem to have been usually produced, nor considerable muscular movement avoided. Wolfers found that the consumption of oxygen increased under alcohol; the output of carbonic acid was also increased, but less extensively; the respiratory quotient was usually lowered. From this the inference is drawn that the alcohol is partly oxidized in the body, but that no lessening of the oxidation processes may be attributed to it. But even if more oxygen be consumed it does not follow that the increased oxidation may not be that of the alcohol, and at the same time include a protection of the tissues.

Against these experiments it has been argued that the animals were rabbits and fastened down in an unfavorable manner. On the other hand Zuntz has urged that perhaps the rabbit from living so largely on carbo-hydrates may be especially adapted to show the influence of alcohol in the respiration.

We have also incidental information in a research by Rumpf,[47] who found that in alcohol narcosis the temperature of guinea pigs is much lowered, and its regulation disturbed, and the oxydation as represented in the oxygen consumption greatly reduced. Obviously the conditions were such as to permit no fair inferences concerning the physiological action of alcohol.

Still more recent are experiments in this direction made by Bodländer, Zuntz (with Berdez), and Geppert.

Bodländer [48] worked with a new apparatus, which has the advantage of leaving the animal perfectly free in a small chamber and undisturbed by any operation for the introduction of breathing tubes and the like. Bodländer found the oxygen consumption reduced for two dogs, 12 per cent. and 19 per cent., the CO_2-excretion 11 per cent. and 19 per cent., the same changes for a rabbit being only 3 per cent. and 8 per cent. respectively. There is, then, a small lessening of the respiratory quotient for the rabbit and no such change for the dogs. But as Zuntz has pointed out, in the article mentioned below, Bodländer's dogs had a very high oxygen consumption in the "normal" experiments which leads to the inference that they were more restless than the experimenter supposed (and any muscular action is a most potent factor in the O_2-consumption) and that the influence of the alcohol was to lessen their activity, and consequently to reduce the respiration. Zuntz also objects to the imperfect ventilation of Bodländer's apparatus, and thinks that the lowering of the temperature of the animals would also tend to lower the oxygen consumption, it having been shown by Pflüger and Velten that a reduction of 1° C. lessens this about 5 per cent.

In Zuntz's experiments with Berdez [49] the latter breathed through a carefully fitted tube with valves, the nose being closed. It was thus possible to ensure perfect muscular quiet and examine the course of the respiratory change for twenty minutes at a time. The outcome of the very few experiments which could be made was that "none of the important factors of the respiratory process was lowered." The oxygen inflow rose about 3.5 per cent., the carbonic acid outflow increasing also about 4.5 per cent. The smallness of these figures as compared with those of Wolters is attributed to the small amount of alcohol which Berdez was able to take without causing muscular unrest.

The latest and in some respects the most interesting experiments are those which Geppert [50] has published, while I was collecting the material for this paper. Geppert has a new and very ingenious apparatus for making respiration experiments on man. This renders it possible to exclude many disturbing factors which are met with in experiments on animals and are there often beyond the control of the experimenter. Excluding all cases where the alcohol caused muscular disturbance or produced sleep (since both conditions have nothing to do with the alcohol effect on respiration for itself) it was found in a number of experiments on four men that "the doses of alcohol employed had no important effect on the oxygen consumption." The carbonic acid excretion was not increased; it was either constant or somewhat lessened. In fact the respiratory quotient appears to me to have been in every case slightly lowered. This may be interpreted to mean that the oxidation of the alcohol has protected some other substance from oxidation and lessened the amount of oxygen which appears in the expired air as CO_2. When carbo-hydrates are consumed the oxygen of the CO_2 sent out is equal to the oxygen taken in for the combustion, its respiratory quotient is said to approach 1.00; the combustion of alcohol on the other hand returns in the form of carbonic acid, but two-thirds of the oxygen taken in, it has a respiratory quotient of 0.66.

It will thus be seen that we are not as yet in a position to make very positive assertions concerning the influence of alcohol on the tissue changes as measured by the respiratory gas exchange or in terms of nitrogen excretion. An ingenious advocate could draw arguments for either side from the experimental evidence at our disposal. But we should be extremely cautious in inferring the physiological influence from those very common results of unwise indulgence in alcoholic beverages, and which are clearly pathological. The imperfect nourishment of many drinkers is explicable by the chronic disturbances of the intestinal

[45] Henrijean. Sur le rôle de l'alcool dans la nutrition. Bull. de l'académie royale de Belgique, 3me. sér. v. 1883.

[46] J. Wolfers. Untersuchungen über den Einfluss einiger stickstoff freier Substanzen, speciell des Alkohols, auf den thierischen Stoffwechsel. Pflüger's Archiv, 1883, xxxii, 222.

[47] Rumpf. Untersuchungen über die Wärmeregulation in der Narkose und im Schlaf. Pflüger's Archiv, 1884, xxxiii, 538.

[48] Bodländer. Einfluss des Weingeistes auf den Gaswechsel Ztsch. f. klin. Med. 1886, xi, 548.

[49] Zuntz. Beitrag zur Kenntniss d. Einwirkung des Weingeistes auf den Respirationsprocess des Menschen. Reprint from the Fortschritte d. Med. 1887, p. 1.

[50] Geppert. Die Einwirkung des Alkohols auf den Gaswechsel des Menschen. Archiv f. exp. Path. u. Pharm., 1887, xxii, 367.

tract with the attendant upsetting of digestion and absorption. This, too, is often enough further complicated by a real insufficiency of food either in quality or quantity. The excessive formation of fat with accompanying degenerations often attributed to alcohol is certainly not always due to its moderate use alone (although even this, in some persons favors obesity); unwelcome or dangerous fatness is often enough met with where there is a complete disuse of alcohol.

Concerning the action of alcohol on the muscles, exceedingly little is really known. Nearly all the experimental researches merely show that quite concentrated solutions of alcohol kill muscular tissue, cause rigidity or hasten it, and precipitate or coagulate the albuminous substances. This is not only what might be expected, but also demonstrates actually nothing concerning the behavior of such quantities as would reasonably and probably reach the muscles. It has, for this purpose, no more value than the platform trick of coagulating egg albumin in a tumbler by spirits of wine, which, I am told, some audiences have found to be very impressive.[51] There is, however, some reason to believe that the use of alcohol (certainly when given in rather large doses to healthy persons) does not enable a man to do more hard work with his muscles, at least, for any length of time.[52] This effect is probably complicated by the influence of alcohol on the nervous system.

One of the most delicate reagents which we have for alcohol, or rather, for alcoholic beverages, is the human central nervous system. As a noted anatomist used to say of the external ear, this can be best studied on one's neighbors. It seems hardly necessary to point out that the effect first manifests itself in the cerebral centres, and that this, for a small amount, is a stimulation.

"It is very difficult to say how far the stimulating action depends on the increased circulation through the nervous centres only, or how much of it may be due to the action of the alcohol on the nervous structures themselves. The symptoms of intoxication must, however, be referred to a paralyzing action of the alcohol on the nerve-centres, for, although as intoxication progresses, a diminution in the activity of the cerebral circulation occurs, and the well-nourished brain becomes anæmic, this alone is insufficient to account for the effects we observe. The first of these are weakening of the mental faculties and of the power of coördination. The higher faculties seem to go first, and a man's judgment becomes impaired, while his memory and imagination are still more lively than usual. Then these faculties diminish, and the emotions become more prominent, so that a man is either ready to swear eternal friendship all round, or becomes as anxious for a fight as an Irishman at Donnybrook; is gay, mirthful, and hilarious, or subdued and lachrymose, melting into a flood of tears without any apparent cause. At the same time, the power of coördination becomes impaired. This is most evident in the tongue and legs, the speech becoming thick and indistinct, so that the pronunciation of the words 'British Constitution' becomes next to an impossibility, and locomotion becomes staggering and uncertain. Although loss of the mental faculties and loss of coördination power generally go hand-in-hand, yet either of them may occur a good while before the other, so that persons who seem stupefied by drink may rise and walk with the utmost readiness, while others, who seem perfectly unaffected while sitting, and can discourse on any subject with freedom, will find great difficulty in steering their way from the table to the door." (Brunton.)

Although the course of the influence is clearly perceptible, such attempts as have been made to define or measure it experimentally have not been remarkably successful. The observations of Exner,[53] Dietl and Von Vintschgau,[54] and Kraepelin,[55] seemed to show that the reaction time (that is, the time required to perceive an appointed signal, and make a simple predetermined signal in reply) may be quickened by alcohol or alcoholic beverages, but that there follows ere long a period of slower replies, and that such a slowing occurs rapidly from a large dose.

Some experiments of my own, which will shortly be published, appear to demonstrate that the influence of alcohol in this respect is even more varied than has been supposed, and that no final conclusions can be drawn at present. I can see no valid reason for supposing that alcohol does not quicken mental activity (where there is any to be quickened). The stimulating influence of small quantities of alcoholic beverages is a matter of too common experience among brain-workers to permit a wholesale denial of it. Obviously, this results from a variety of causes, and is subject to enormous individual variations and limitations, and the bad effect of large quantities is beyond question. It will not do to urge, as is usually done, that mankind was mistaken as to the influence of alcohol on the body-temperature, and so must be about the effect on the brain. A man's knowledge of his temperature is exceedingly limited, quite subjective, affected by many influences, and altogether uncertain; he merely knows that he feels warmed. But if we test the temperature by a thermometer, why may we not judge somewhat of the mental effect by the work which is done under such circumstances? Looked at in this way, there is experience enough to warrant us in saying that very often men really do think better and write better with moderate alcoholic stimulation than without it. It does not in the least follow from this that constant stimulation for such a purpose is good or permissible any more than it does that a man should always run when he can just as well walk, although progression would thus be more rapid for a time.[56]

[51] Engelmann (Ueber die Flimmerbewegung, 1868, p. 58, and in Hermann's Handbuch der Physiologie, I, 402) has shown that small doses of alcohol increase the frequency and energy of ciliary motion, while large doses bring it to a standstill. Dogiel, too, asserts that the force of muscular contraction is similarly affected. His paper (Pflüger's Arch., viii, 606) is, however, but an abstract so brief and aphoristic, that it affords no chance of judging of its true value.

[52] Parkes, in the first paper quoted above, tested the muscular power of his soldier (probably with a dynamometer), and found that no change was produced by alcohol. In one of the later series of experiments, where the soldier labored hard with the spade, he worked (in his opinion) very much less well with brandy than without it, but the quantities which led to this result were considerable. This view is also favored by his paper in the Lancet (1874, ii, 238, 263, "Report on the Issue of a Spirit Ration during the march to Coomassie,") which presents evidence that the use of alcohol does not, on the whole, enable soldiers to endure more fatigue, except when taken *after* the work of the day is done. But even here, the evidence is not altogether convincing to those who are not professional temperance agitators.

[53] Exner. Pflüger's Archiv, 1873, vii., 628.

[54] Dietl und Von Vintschgau. Pflüger's Archiv, 1878, xvi, 316.

[55] Kraepelin. Wundt's Philosophische Studien, 1881-83, I, 573.

[56] As to any special action on nerves and nerve-centres, there may be added the declaration of Dogiel that the irritability of motor and of sensory nerves is, at first, increased and then diminished, and that the reflexes of beheaded frogs are, at first, quickened and then slowed. On the other hand, Meihuizen (Pflüger's Archiv, 1873, vii, 217) found that these reflexes were much slowed at first, and for a considerable period, being followed by a considerable quickening, but he got these results with what must be considered a large dose, and got less constant ones when the quantity of alcohol was only half as great. Kremer's observation (Pflüger's Archiv, 1884, xxxiii., 288) that

But I am wandering too much from my original purpose, yet I trust that the effort not merely to examine some of the older experimental foundations for an opinion about alcohol, but also to inquire what the most recent work has done to strengthen or to weaken them may not be altogether unwelcome to any reader who may chance to have followed me thus far. I hope that I have made it clear that the statements of Prof. N. S. Davis, so far as they have to do with the physiological action of alcohol, are not in accord with the knowledge available when the tract was printed, and are also not supported by the more recent investigations. The points which I should like to insist upon, in recapitulation, are these :

(1) That alcohol enters the animal body readily, and is there very completely disposed of.

(2) That the oxidation of such a substance may fairly be expected to develop force, and that although we cannot as yet say just how this takes place and just what it accomplishes, we have no right to deny that it does occur.

(3) Our knowledge, such as it is, of the influence of alcohol on the muscular and nervous structures of the body, on the function of digestion and absorption, on the heart and circulation, on the heat-production and regulation, on the tissue changes (the metabolism) of the body, and on the psychic functions, does not warrant us in saying that the really moderate use of alcohol is harmful, not to say dangerous, from the point of view of the physiologist.

But we are told, " Wine is a mocker, and whoso is deceived thereby is not wise." Well, there are a good many " mockers " nowadays. Beef tea is one of them. Various artificial foods and similar preparations belong to the same category. It is an exceedingly unfortunate thing, doubtless, to be deceived by these things, but I cannot help feeling that it is a much worse thing to be a deceiver concerning them. I have an impression that the same " wise man " has also urged men " to buy the truth and sell it not." There are times, perhaps, when it is well not to tell the whole truth, but I have yet to learn how the human race can be benefited, in the long run, by systematic deception and by the wholesale circulation of what is, to say the least, not true. I see no reason why alcohol should be made an exc ption to this principle.

I am, I trust, fully awake to the dangers of alcohol, to the harm which it has done in the past and continues to do in the present. If any one shall choose to find in this article a warrant for the indiscriminate and heedless use of alcoholic stimulants, the responsibility must rest with him. It is altogether foreign to my purpose to examine the wide field which a consideration of the evil alcohol may do, would open before us. There are some considerations, however, which may find a place here.

We must not forget that although alcoholic beverages may supply the place of food for healthy persons they can ordinarily do so only to a very limited extent. The exceptional cases are far too infrequent to afford the slightest warrant for substituting alcohol for accepted foods or for any portion of them, and such a course is almost absolutely certain to result in harm to the body. In this sense, there is probably rarely or never any *necessity* for the regular use of alcohol, even in a moderate degree. Therapeutically, there can be no question that many disturbances of function are favorably influenced by a reasonable use of alcohol, but even here, as good results may be frequently obtained by other means, always provided that the habit of taking medicine or considering one's self an invalid be not encouraged, which is for some people a much worse habit than a moderate indulgence in alcohol would be.

The chief reason why men all over the world take alcohol, is that they like its effects, that it makes life pleasanter, that it smooths the rough places. A few take it to drown their sorrows, to forget a trouble or a difficulty which it would doubtless be much better to meet bravely and overcome, if they could, but this is not the reason of its common use. Men like the cerebral stimulation, and the vast majority care for no further action from what they drink, and this majority can attain this effect without going further and without harm to themselves. I am well aware that very many, unfortunately, do not stop at this point. It is but too true that not merely one man's meat, but also one man's drink, is another man's poison, and some men should refrain from alcohol, just as some men should refrain from lobster, or mushrooms, or oysters, or salmon.

The temperance movement of the future, if at some time it shall have become a true temperance movement, as I yet hope it will, the temperance movement of the future will have to recognize that the field for its activity lies not in the dissemination of falsehood about what alcohol is and does, but in the control of its rational use and in the prevention of all abuse. Here is an immense and fruitful field, and were but half the energy consumed in tilling it, which has been spent in reckless denunciation and misrepresentation, the evils of intemperance would have been largely diminished. Intemperance is a terrible weed, but its roots will be found to be entangled amid many social problems of heredity, poor food, over-work, bad cooking and bad homes, all quite as important, if indeed not more important, than the question of alcohol. And as to alcohol itself, finally, there is much to do in the regulation of its quality. It has been demonstrated again and again that the harmfulness of alcoholic beverages is very commonly due to impurities which can be avoided or greatly reduced. The "wise man" in recording the ripe results of his great experience, but anticipated modern research in attributing woe. sorrow, contentions, wounds without cause, and redness of eyes, not merely to "those that tarry *long* at the wine" but also to "those that go to seek *mixed* wine."

The admixture of other so-called higher alcohols, besides the ethyl alcohol, which we have been considering in this paper, is a source of much danger. Experiments have shown thoroughly that the evil effects of ethyl alcohol in large quantities, are produced by the higher alcohols in far smaller doses. This is an interesting and very important part of the subject, but even a brief consideration of it would lead us too far.

alcohol lowers the sensibility of the skin for points, seems to rest upon one single experiment with sixty grammes of brandy.

— A physician writes to the *Medical Press*, " within the last five years, in a district embracing sixty square miles or so by the sea, I have noted the hour and the minute of no less than ninety-three demises in my own immediate practice, and every soul of them all has gone out with the tide, save four who died suddenly by fatal accident."